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AUTHOR Ghatala, Elizabeth S.; And Others
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ABSTRACT

Second-grade children were given general training in monitoring the utility of strategies, the affective consequences of strategy use, or no strategy-monitoring training. They then performed an associative learning task, first without strategy instructions and then with instructions to use either an effective or ineffective strategy. All training conditions produced short-term maintenance of the effective strategy, but only the strategy-utility training resulted in long-term maintenance. Subjects given strategy-utility training abandoned the ineffective strategy at a higher rate than children given strategy-affect or no training. Responses to metamemory questions indicated that only in the strategy-utility condition was strategy efficacy a prime consideration in strategy-use decisions. This experimental evidence bolsters the case for including monitoring instruction in multicomponent training packages aimed at producing durable strategy use. (Author)

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Training Cognitive-Strategy Monitoring in Children

Elizabeth S. Ghatala

University of Houston, University Park

Joel R. Levin

University of Wisconsin-Madison

Michael Pressley

University of Western Ontario

Marguerite G. Lodico

Albany, N.Y.

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Address correspondence to:

Elizabeth S. Ghatala

Department of Educational Psychology

College of Education

University of Houston - University Park

Houston, TX 77004

(713) 749-7621

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Abstract

Second-grade children were given general training in monitoring the utility of strategies, the affective consequences of strategy use, or no strategy-monitoring training. They then performed an associative learning task, first without strategy instructions and then with instructions to use either an effective or ineffective strategy. All training conditions produced short-term maintenance of the effective strategy, but only the strategy-utility training resulted in long-term maintenence. Subjects given strategy-utility training abandoned the ineffective strategy at a higher rate than children given strategy-affect or no training. Responses to metamemory questions indicated that only in the strategy-utility condition was strategy efficacy a prime consideration in strategy-use decisions. This experimental evidence bolsters the case for including monitoring instruction in multicomponent training packages aimed at producing durable strategy use.

Training Cognitive-Strategy Monitoring in Children

In a recent investigation, Lodico, Ghatala, Levin, Pressley, and Bell (1983) demonstrated that training second-grade children to attend to the effects of strategies on their task performance (i.e., providing strategy-monitoring training) before teaching them specific memory strategies enabled second-grade children subsequently both: (a) to select a more effective over a less effective memory strategy; and (b) to justify their selection of the strategy in terms of its effectiveness. Lodico et al.'s findings add to a growing body of evidence in support of the notion that subjects' strategy "metacognitions" (e.g., Flavell, 1981)--subjects' knowledge about strategies--will guide their subsequent strategy use (see, for example, Pressley, Borkowski, & O'Sullivan, in press). Moreover, in contrast to previous studies (e.g., Brown & Barclay, 1976), Lodico et al. were able to separate metamemory training (which provides general procedures for gaining knowledge about strategies) and memory strategy instruction (which provides information about specific strategies), thereby allowing a clearer determination of the effects of metamemory training per se.

It is useful for both theoretical and practical reasons to pursue the approach of strategy-monitoring training as a technique for influencing subjects' use of strategies. On the theoretical side, this approach provides direct experimental evidence (as opposed to correlational evidence) concerning the link between metamemory variables and strategy use (e.g., Borkowski, in press; Kendall, Borkowski, & Cavanaugh, 1980; Moynahan, 1978, Waters, 1982). On the practical side, exploring ways of helping children to acquire metamemory knowledge about strategies is of particular importance inasmuch as, relative to adults, children's metamemory is not as likely to grow merely as a function of gaining experience in using strategies (Pressley, Levin, & Ghatala, in press). Furthermore, although numerous studies have reported increases in children's strategy use following provision of feedback about strategy effectiveness (e.g., Black & Rollins,

1982; Borkowski, Levers, & Gruenenfelder, 1976; Cavanaugh & Borkowski, 1979; Kennedy & Miller, 1976; Kramer & Engle, 1981; Paris, Newman, & McVey, 1982; Pressley et al., in press; Ringel & Springer, 1980), this kind of approach is limited to the extent that an external agent must provide information about each specific strategy the learner may encounter. Apparently this limitation can be overcome by the training procedures devised by Lodico et al. (1983), which enables children to ascertain on their own the effectiveness of strategies. However, additional work is needed to clarify the effective components of that procedure. Such was the purpose of the present study.

The present study extended Lodico et al. (1983) by examining variations in the criterion task and in strategy-monitoring training. The criterion task procedures studied here resembled more closely the procedures employed in most strategy-instruction research wherein subjects acquire a single specific strategy rather than two strategies differing in potency. The major question of interest in such strategy research and in the present study is whether subjects will maintain a single trained strategy when external prompts are removed. Thus, following strategy-monitoring training children were given three different paired-associate lists. The first list for all children constituted a baseline list, on which subjects were instructed in general task procedures but were given no strategy instruction. On the second list, half of the children were taught an effective sentence elaboration strategy, and half were taught an ineffective counting strategy. Although instruction in a strategy known to be ineffective is not a typical feature of strategy-instruction research, its inclusion here permits rejection of the rival hypothesis that children's selection of an instructed strategy represents simply a "demand characteristic" (Orne, 1962) associated with the desire to please or obey an adult authority figure. Immediately following the second list, subjects were given a third list and told that they could learn the pairs in any way they wished, with the goal of remembering as many pairs as possible. Subjects then indicated the strategies used on the third list and explained their choices. In addition, several delayed tests of strategy

maintenence were given to those subjects who were initially taught the effective strategy.

Strategy-monitoring training was varied in the present study as well. Prior to the three paired-associate lists, one group of children received the strategy-monitoring training devised by Lodico et al., training that focused children's attention on the usefulness or utility of the strategy for improving performance. A second group of children was given instruction and practice in monitoring affective aspects of strategy use (viz., assessing how much fun strategies were to use). A control group was given no training in monitoring strategy use. The inclusion of the strategy-affect training was dictated by a couple of concerns. First, in contrast to the no-training control condition, this condition requires reflection upon and judgments about strategic behavior during training similar to the strategy-utility condition. Thus, general strategy-monitoring effects are controlled, and comparison of the two strategy-monitoring variations permits separation of utility-relevant and utility-irrelevant aspects of strategy monitoring. Because strategy use depends critically on knowing that a strategy facilitates remembering (e.g., Borkowski et al., 1976; Kennedy & Miller, 1976; Paris, 1978; Paris, Newman & McVey, 1982), training in monitoring strategy utility should produce better maintenance of an effective strategy (and abandonment of an ineffective strategy) than training in monitoring strategy affect. On the other hand, the effects of affect on maintenance have not been studied previously, and thus, there existed the a priori possibility that awareness of one's affective state when using a strategy may contribute to strategy maintenance.

Method

Subjects

The subjects were 126 second-grade children (mean age = 7.6 years; range = 7.2 to 8.0 years) from several elementary schools located in middle-class suburban

neighborhoods in a southwestern metropolitan area of the United States. A block-randomization procedure was used to assign 21 children to each of the six conditions of the experiment.

Materials

Three paired-associate lists, each consisting of ten concrete noun pairs, were constructed. Within each condition, seven subjects apiece received a given list as the first, second, or third list presented. Two additional lists, one consisting of five pairs and one of ten pairs, were used to assess long-term maintenance of the effective strategy. A practice list of three pairs was also constructed.

Procedure

The children were tested individually in a private room in the school building by an adult female experimenter.

Strategy-utility condition. Prior to receiving the three paired-associate lists, the children in the strategy-utility condition were given the following training. Preliminary instructions suggested to the children that there are many ways to play games and that some ways are better than others. In addition, they were told that in order to play a game well, they must use the method that allows them to do better. The children were then given two examples. The first example involved drawing a circle, with children required to try two different methods, free hand and tracing a circular cookie cutter. They were then asked to determine which time they were able to draw a circle better and why they thought they did better. Then they were asked what they would do if they were asked to draw a circle again and they wanted to draw it the best they could. Feedback was provided, and those children having difficulty (few did) repeated the game. The children were praised for correctly answering the questions and were reminded that ". . . keeping track of how you are doing when you play a game helps you to choose the best way to play." The second example required memorization of a list of letters initially presented in random order. The children

studied the letters, and then attempted recall. Following this, the children were instructed to arrange the letters so as to spell their names (each child received a set of letters selected to spell his/her name). After the letters were removed, recall was again requested. After the second recall test, the children were asked on which occasion they had remembered more letters, why, and what they would do if they were to play the game again. Again, all children received verbal reinforcement and a reminder about monitoring their performance.

Strategy-affect condition. The children in the strategy-affect condition were told at the outset that there are many ways of playing games and that some ways are more fun than others. They then played the circle-drawing game and were asked to determine which time they had more fun, why they had more fun, and which way they would draw the circle if they were to play the game again. The children were praised for selecting either method as "more fun" and were reminded that ". . . some ways of playing games are more fun than others." The children then played the letter game followed by the same questions, verbal reinforcement and reminder.

Control condition. The children in the control group spent an equivalent period of time playing games with the experimenter, during which they practiced the same strategies on the same tasks as the experimental children. However, the control children received no instruction about monitoring any aspect of the strategies or their performance.

At the end of the training or placebo control period, all children were told that they were going to play another game that involved remembering pairs of words. They were told to do their best to remember as many words as possible. Children were then given the three paired-associate lists. On the study portion of each list. ten noun pairs (printed on 12.7 X 20.3 cm cards) were presented at 10-second intervals. The experimenter read the words on the cards, and then placed them in front of the

children. On the test trials, the stimulus words were presented one at a time and the children were asked to recall the response words. The test trials were subject-paced.

Before the first list, children were given general instructions for the paired-associate task, along with a three-pair practice list to illustrate study and test procedures. The children were not instructed in a strategy but were told to do whatever they wanted to help them remember as many pairs as possible. Following testing on the first list, the children were told they would play the game a different way. Half of the children were told to remember the pairs by "making up a story" (sentence) connecting the words; the experimenter demonstrated this with a sample pair. A sentence strategy is known to improve markedly children's paired-associate learning (Levin, 1976; Rohwer, 1973). The children practiced the sentence strategy on one of the practice pairs, and then proceeded to the actual study/test trial. For the other half of the children, the experimenter described and demonstrated a counting strategy. This strategy consisted of counting the letters in the stimulus and response words of each pair, and was known from pilot testing to produce a marked decrease in performance from the baseline level for second-grade children. The children practiced their instructed strategy, and then were presented with the second list to learn. All children were required to perform their instructed strategy overtly on the study portion of the second list.

Immediately following the second list recall trial, the children were told that they would play the game one more time and that they could play the game however they wished. It was emphasized that they were to try to remember as many pairs of possible. As on the baseline trial, overt responding during the study portion of the trial was neither required nor encouraged.

After testing on the third list was completed, the experimenter pointed to that list and said, "You were just tested to see how well you remembered these pairs. I watched as you learned each pair, and I believe I know what you did to remember

each one. Now I want to see if you can tell me what you did for each pair. Most boys and girls your age who I have talked to have told me about various ways to remember the pairs." The experimenter then went on to describe four strategies: repeating the words in a pair; reading the words carefully; making up a sentence (a "story") for the words in a pair; and counting the letters in each word. Each strategy was accompanied by a card containing a picture of a stick figure looking at the pair, dog-hat, with a balloon caption showing the figure executing the strategy "in his head." The subject was then told, "Now I am going to show you each of the pairs that you just learned, and you show me which of these things you did to learn the pair. You can point to one of these cards. If you did not do any of these things for a pair, just tell me what you did." Both the order in which the strategies were described and the order of the pictures displayed in front of the child were varied randomly. After the children indicated their strategies for the ten pairs, they were asked to explain why they used those strategies. The experimenter first asked about the subject's most frequent strategy; "I noticed that you [described strategy] for many pairs. Why did you do this?" Following the subject's free response to this probe, the experimenter gave a forced-choice probe, "Here are three reasons why you might have done that. Which reason tells best why you did it? You thought it would help you remember better, it was fun; it was an easy one to use." The order of the alternatives was varied randomly. Both the free-response and forced-choice probes were repeated for any strategy the subject reported for at least three pairs.

Finally, the experimenter displayed the first two lists and said, "The first time you played the game you learned these pairs, and the second time you learned these pairs. Which time did you remember more pairs? Why did you remember more pairs that time?" The children in the effective-strategy conditions were then thanked for their participation and returned to their classrooms. The experimenter spent extra time with the children who were taught the counting strategy during which she explicitly

attributed the children's poor performance on the second trial to the counting strategy. Then, to end the session on a positive note, the children were instructed in the sentence strategy.

One week following this initial session, children in the effective-strategy conditions returned to the experimental room. They were reminded of the memory game and the goal of remembering as many pairs as possible. They were then presented with five new pairs to study. Instead of a test trial, subjects were asked to indicate the strategy used for each pair by pointing to one of the picture cards previously described. Approximately eight weeks following this session (nine weeks following original strategy instruction) selected children were presented with a 10-pair list to study. Immediate recall of the pairs was assessed, and then the children were asked to indicate the strategy used for each pair.

Results

The data were analyzed separately for each of the assessment measures (the three initial lists and the two delayed lists). The design consisted of three levels of training (strategy utility, strategy affect, and control) crossed with two types of instructed strategy (effective and ineffective). Because questions of interest focused on comparisons of training conditions within strategy types, a traditional two-way analysis of variance was bypassed in favor of six more direct planned comparisons (the three pairwise comparisons between training levels within both the effective and ineffective strategy types). The three comparisons within each strategy type were assessed via directional comparisons based on a per-comparison Type I error probability of .0167 and a familywise Type I error probability of .05--see Marascuilo and Levin's (1983, pp. 353-354) discussion of the Dunn-Bonferroni procedure in this context. Both comparisons of means (based on t statistics) and comparisons of proportions (based on Z statistics) followed this approach. The recall results for the initial session are summarized in Table 1.

Insert Table 1 about here

List 1 (Baseline)

In both the effective and ineffective strategy conditions, subjects who received strategy-utility training performed slightly better than control subjects on the paired-associate task (see Part A of Table 1). In both cases, the performance advantage was statistically significant, $\underline{t}$s = 2.73 and 2.56 for the effective and ineffective strategy conditions, respectively. As may also be seen in Table 1, in the ineffective strategy conditions, the mean performance of strategy-affect subjects was exactly equal to that of strategy-utility subjects, and statistically higher than that of controls, $\underline{t}$ = 2.56. The mean performance of strategy-affect subjects in the effective strategy condition did not differ reliably from that either control subjects, $\underline{t}$ = 1.88, $\underline{p} > .0167$, or strategy-utility subjects, $\underline{t} < 1$.

Thus, in both the effective and ineffective strategy conditions, strategy-utility training produced higher recall than in the control condition on the baseline trial (i.e., following training only on general strategy monitoring, and not on specific strategy use for the paired-associate task). Though perhaps somewhat surprising, Lodico et al. (1983) found a similar difference in baseline recall favoring strategy-utility trained subjects over control subjects. Those authors speculated that the training increased the children's general level of attentiveness and/or motivation on the memory task. In the present experiment, however, it is important to note that the lack of difference in recall between strategy-utility subjects and strategy-affect subjects suggests that whatever general motivational effect was present was comparable in the two monitoring treatments. This was true in both the effective and ineffective strategy conditions, and should be remembered when interpreting subsequent results.

List 2 (Strategy Instruction)

On the second list of the initial session, subjects were instructed to employ either a sentence strategy (effective conditions) or a counting strategy (ineffective conditions) to remember the paired-associate items. As is immediately apparent from Part B of Table 1, the two strategies worked precisely as intended and in distinctly opposite directions. The sentence strategy elevated students' performance in all conditions. Indeed, all 63 subjects experienced a performance increase from the first list, with no child recalling less than 8 out of 10 list items. In symmetrical contrast, all 63 counting strategy subjects experienced a decrease in their recall relative to the first list, with none recalling more than 2 out of 10 items. All six of the within-strategy type comparisons involving training were statistically negligible, all $\underline{t}$s < 1. Of course, ceiling and floor effects preclude meaningful statistical statements, other than the statement that the sentence strategy was highly and equally effective for all training conditions on this 10-item list, and that the counting strategy was highly and equally ineffective for all training conditions.

List 3 (Immediate Maintenance)

Recall. Performance on the immediate maintenance trial (List 3) is summarized in Part C of Table 1. The mean recall of strategy-utility subjects exceeded that of control subjects in both the effective and ineffective strategy conditions, $\underline{t}$ = 4.04 and 4.34, respectively. At this point, however, such differences cannot be attributed to effects of training on strategy maintenance inasmuch as similar differences between the two conditions were found on the baseline trial (List 1)--see Part A of Table 1. On this list as well, strategy-affect subjects outrecalled control subjects in the effective strategy conditions, $\underline{t}$ = 2.99. Of most interest here, however, was the finding that in the ineffective strategy conditions strategy-utility subjects surpassed strategy-affect subjects in their recall, $\underline{t}$ = 4.19--quite in contrast to the List 1 results where the two strategy means were identical. There was no mean performance difference between

strategy-utility and strategy-affect subjects in the effective strategy conditions, t = 1.05.

An important aspect of the List 3 results is the following: In the three effective strategy conditions, subjects appear to have maintained their newly learned sentence strategy, as evidenced by the substantial increase in recall from List 1 (baseline) to List 3 (maintenance). In particular, all subjects in the effective strategy conditions improved their performance by at least two items. In contrast, in the ineffective strategy conditions, the comparability of recall performance between Lists 1 and 3 suggests that subjects were abandoning their newly acquired counting strategy and returning to whatever they were doing spontaneously on the baseline trial. Although it is possible that the List 1 to List 3 improvement by subjects in the effective strategy conditions is due to simple practice effects per se, the corresponding lack of improvement in the ineffective strategy conditions complicates such an argument. These and the other results in this section will be interpreted in conjunction with subjects' strategy reports, which follow.

Interview data: Effective strategy conditions. In the effective-strategy conditions, virtually all subjects reported having maintained the sentence strategy on List 3. Except for three subjects (one in the strategy-affect monitoring condition and two in the control condition) who reported using the strategy for nine out of ten pairs, subjects indicated using the strategy for all ten pairs. This finding suggests that the previously noted recall differences among conditions on List 3 may also be attributed to a general motivational effect of the strategy-monitoring training rather than to differential use of the sentence-generation strategy. Moreover, the finding that List 3 performance in the effective strategy conditions was somewhat lower than the corresponding List 2 performance does not appear to be attributable to a concomitant decrease in strategy usage by subjects (i.e, it may not have resulted from a lack of complete strategy maintenance). A more likely interpretation is that the required overt

execution of the sentence strategy on List 2 was replaced by an elected covert execution of the strategy. Inasmuch as external versions of a strategy have been consistently found to be more reliably executed than the analogous internal version of the same strategy (e.g., Levin, 1976; Pressley, 1977; Rohwer, 1973) the present finding is not surprising.

Reasons for children adopting the strategy they did are summarized in the top portion of Table 2. Conclusions regarding subjects' reasons for strategy selection on List 3 are similar whether the query called for a free response or a forced choice among alternatives. Thus, only the latter data are reported.[1]

Two things should be noted in regard to the free-response data, however. First, when asked why they chose the strategy they did, strategy-utility subjects typically did not "parrot back" the same words used by the experimenter during training. They said such things as: "to put the words together in my mind," "I learned how to remember the words together with a story," and "to remember--to pound it in (hitting head with hand)." This suggests that the initial (potentially detached) experimenter's instruction was transferred to the subject's personally usable knowledge. Second, in the free-response protocols it was clear that when a subject gave "because it was easy" as a reason, "easy" referred to easy to execute the strategy rather than easy to remember from it. This was ascertained via follow-up questions each time an "easy" response was given. Moreover, on the forced-choice task, the category "easy" was carefully described to the child in "easy-to-execute" terms. Thus, we can be quite certain that subjects were interpreting the term "easy" as strategy-execution effort--a memory-unrelated factor.

It may be seen that all of the subjects in the strategy-utility condition indicated that they had used the sentence strategy because they thought it would help them remember better ("Memory" in Table 2), whereas none of the subjects in the two other conditions did, both $\underline{Z}s = 6.48$. The majority of subjects in those conditions indicated

that they had applied the sentence strategy because it was fun to use. All subjects in all conditions were aware that they had remembered more pairs on List 2 (strategy instruction) than on List 1 (baseline), which is not surprising given the magnitude of the recall increase from List 1 to List 2 (see Table 1). However, whereas all strategy-utility subjects attributed this increase to their use of the sentence strategy, all but one (95%) of the subjects in each of the other conditions offered strategy-unrelated explanations (e.g., "fun," "easy," "don't know") for their improved performance, both $\underline{Z}$s = 6.18.

Insert Table 2 about here

Interview data: Ineffective strategy conditions. In all ineffective strategy conditions, the majority of subjects tended to abandon the counting strategy on the List 3 maintenance trial. It was, however, clear that a higher proportion of subjects chose abandonment in the strategy-utility condition than in the other two. Parametric comparisons based on mean numbers of items are precluded because of distributional abberations (viz., two-thirds of all subjects reported not having used the counting strategy for any of the 10 list items, which produced extreme positive skewness). At the same time, it should be noted that all (100%) of the strategy-utility subjects applied the counting strategy to no more than 2 out of 10 list items, in contrast to 57% and 62% of the subjects who did so in the strategy-affect and control conditions, $\underline{Z}$ = 3.38 and 3.14, respectively. Thus, the strategy reports following List 3 parallel the earlier reported recall data for this list. That is, relative to strategy-utility subjects, strategy-affect and control subjects reported greater usage of the counting strategy on this list, and their inferior recall performance reflected this. The same interpretive problem exists here as was apparent in the effective strategy conditions, resulting from the baseline (List 1) recall differences between strategy-utility and control subjects. Because

of this, List 3 differences cannot be attributed solely to differential abandonment of the counting strategy. Given the complete equality of the two strategy-monitoring groups on the baseline list, however, the List 3 differences in that case can reasonably be attributed to such differential abandonment. As further indicative evidence, across all subjects, the correlation between the number of pairs for which a counting strategy was reported and number of items recalled was a fairly substantial r of -.62 suggesting that adopting the counting strategy was accompanied by lower recall.

The reasons behind children's List 3 strategy selections are summarized in the bottom portion of Table 2. For each child it was possible to identify the most frequently used strategy. Typically, subjects who reported using the counting strategy for 0-3 pairs reported as their most frequent strategy either rote repetition or reading carefully. For subjects reporting the counting strategy for 8-10 pairs, counting of course was the most frequent strategy. For subjects reporting the counting strategy for 4-7 pairs, one of the strategies (counting, rote repetition, or reading carefully) was reported more frequently than any other. As may be seen from Table 2--and as was true in the effective strategy conditions--a much higher proportion of subjects in the strategy-utility condition than in each of the other conditions offered a memory-related reason for their strategy selections, both Zs = 4.72. And, although subjects in all conditions were highly accurate (ranging from 81% to 100%) in assessing that they had remembered more on List 1 (baseline) than on List 2 (strategy instruction), once again strategy-utility subjects traced their differential performance directly to the counting strategy. Among those children who correctly assessed that they had remembered more items on the baseline trial, all (100%) strategy-utility subjects mentioned the counting strategy in their explanations of the recall difference, in contrast to only 22% and 37% of the subjects mentioning this in the strategy-affect and control conditions, Z = 5.05 and 4.35, respectively. Specifically, all of the children in the strategy-utility condition "blamed" their poorer List 2 recall performance on the counting strategy whereas the

majority of students in the two other conditions did not connect the decrease in recall with the counting strategy.

List 4 (One-Week Maintenance)

One week following the initial session, subjects in the effective strategy conditions were given a new five-pair list to study, and then (without recalling them) were asked to indicate which of the four experimenter-described strategies they had used for each pair. The average numbers of pairs for which subjects reported having used the sentence strategy were 4.67, 3.24, and 3.67 in the strategy-utility, strategy-affect, and control conditions, respectively. Because of the near ceiling-level reports (and, consequently, severely restricted variance) in the strategy-utility condition, parametric comparisons among means were not conducted on these data. However, comparisons of proportions indicated that a substantially higher percentage of strategy-utility subjects (90.5%) applied a sentence strategy to to at least four out of five pairs than did either strategy-affect subjects (52.4%) or control subjects (61.9%), Z = 2.73 and 2.17, respectively. Thus, it can be concluded that strategy-utility subjects maintained the sentence strategy to higher degree, relative to subjects in the other two groups, on a new list administered a week following training.

List 5 (Nine-Week Maintenance)

Approximately eight weeks after the one-week maintenance (nine weeks after the initial session), ten subjects from each of the effective strategy conditions were selected for one final maintenance assessment. All subjects were ones who had reported having used the sentence strategy for at least four out of five pairs on the one-week maintenance list (List 4). The ten most recently tested subjects in each condition who satisfied this criterion were the ones selected. A 10-item study list was administered, followed by a recall trial and strategy reports.

Strategy reports. The average number of pairs (out of 10) for which subjects reported using the sentence strategy were 9.8, 7.1, and 6.2 in the strategy-utility,

strategy-affect, and control conditions, respectively. Again, the ceiling-level reports of strategy-utility subjects precluded the valid conduct of parametric comparisons of means. Moreover, large-sample (Z) comparisons of proportions were not warranted as a result of the small sample sizes here (N = 10). Consequently, the proportions of subjects reporting having used a sentence strategy for at least 8 out of 10 list items were compared using Fisher exact probabilities in conjunction with the Dunn-Bonferroni procedure. All (100%) of the strategy-utility subjects reported using the sentence strategy for that many items, in comparison to 50% in each of the two other conditions--a significant difference (p = .0162).

Recall. Memory performance of the nine-week maintenance trial paralleled the strategy-report data. That is, recall in the strategy-utility condition (a mean of 83% correct) was statistically higher than that in either the strategy-affect condition (61%), t (27) = 2.93, or the control condition (59%), t (27) = 3.20. It should be remembered that the strategy-utility and control conditions differed in memory performance on the baseline list (List 1), as well as on the immediate maintenance (List 3) when apparently there was no difference in strategy usage reported by subjects in these conditions. Thus, interpretation of the recall difference on the nine-week maintenance list (List 5) in terms of differential strategy usage is somewhat equivocal. No such ambiguity clouds interpretation of the recall difference favoring the strategy-utility over the strategy-affect group; however, in that memory, performance in these two conditions was equivalent on earlier trials (as were strategy reports). Thus, not only did children in the strategy-utility condition report more use of the sentence strategy than did children in the other conditions; their performance indicated that they actually used the strategy more often.

Further validation of the strategy reports was gained by correlating subjects' recall scores on the nine-week maintenance list with the number of pairs for which they reported using the sentence-generation strategy. The strong, positive correlation

(r = .84), indicates once again that subject's reports of strategy use seem to represent a valid predictor of their memory performance (see also Pressley, Levin, & Ghatala, in press; and Pressley, Levin, Kuiper, Bryant, & Michener, 1982).

Discussion

What happened in this study? All children executed a memory strategy after receiving instruction; relative to baseline performance all children experienced a dramatic change in performance while using the trained strategy. Also, most children were cognizant of the performance shift. Yet metamemory knowledge acquired from this experience differed as a function of monitoring training. In their responses to the metamemory probes, children in the strategy-utility training condition clearly articulated the link between use of the strategy during initial training and changes in performance as well as the link between use (or nonuse) of the instructed strategy and the goal of remembering on subsequent maintenance trials. In contrast, children in the strategy-affect and control conditions did not articulate these linkages as clearly.

The clearer conceptualization of the linkages between performance, strategies, and goals after strategy-utility training was accompanied by enhanced long-term maintenance of the effective strategy as well as by more ready abandonment of the ineffective strategy. With respect to this latter finding, one factor that may well promote continued use of a strategy is children's desire to please the adult authority figure who taught them the strategy. The present strategy-utility monitoring training effectively focused children on performance aspects of strategy usage so that situational demand characteristics did not influence their strategy decisions.

Although theoretical statements about strategy monitoring-metamemory relationships abound in the literature, there are very few studies that include unconfounded examinations of strategy instruction and monitoring instruction (e.g., Lodico et al., 1983). When the data presented here are combined with the Lodico et al. (1983) results, there is solid evidence that monitoring training per se makes an

important contribution to efficient strategy instruction. This evidence bolsters the case for including monitoring instructions in multicomponent strategy training packages aimed at producing durable strategy use (e.g., Brown, Campione, & Barclay, 1979; Day, 1983; Palinscyar & Brown, 1983; Schleser, Meyers & Cohn, 1981).

A question that follows from this study and others recently completed in our laboratory concerns the conditions under which self-derived knowledge of strategy efficacy is used as a basis for strategy decisions. Children in the strategy-utility condition employed their knowledge of strategy efficacy to guide their choice of strategies. However, that children often do not utilize self-derived strategy-efficacy information is clear from two other studies. In a recent study by Pressley, Ross, Levin, & Ghatala (1983), fifth and sixth graders practiced two different memory strategies, one much more potent than the other. Although most children realized the differential strategy effects, the minority used this information to make a subsequent strategy decision unless given a specific prompt to do so. Children's failures to use strategy-efficacy information spontaneously is a robust effect, apparent in the child data collected by Pressley, Levin and Ghatala (in press) as well. Fortunately, the strategy-utility training studied here overcame this deficiency. In this regard, strategy-utility training procedures provide practice not only in (1) monitoring one's performance and (2) inferring causal connections between strategies and changes in performance (strategy-efficacy knowledge), but also--and perhaps critically for the question under discussion--in (3) selecting a strategy based on strategy-efficacy knowledge. The first element of training, monitoring performance is something that young children seem to do spontaneously in the circumstances addressed in this study--the control children were quite proficient in assessing their performance (see also, Lodico et al., 1983; Mohnahan, 1978). Yet as may be seen from the present control condition results, young children do not automatically derive strategy-efficacy knowledge from monitoring their performance. The second component of the strategy-utility training insured acquisition

of such metamemory knowledge and, thus, is an example of what have been called "metastrategies" (Chi, in press) and "Metacognitive Acquisition Procedures" (MAPs)--see Pressley, Borkowski, and O'Sullivan (in press). A reasonable conclusion based on the Pressley et al. (1983) and Pressley, Levin and Ghatala (in press) results is that metacognitive acquisition procedures that result in strategy-efficacy knowledge may not be sufficient to guide children's decisions unless something is done to ensure active utilization of that knowledge. That "something" was likely provided by the third component of the present strategy-utility training, which explicitly oriented children to use the strategy-efficacy information in decision making.

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Footnote

[1]The free-response data were scored "blindly" by two independent judges. There was 100% agreement concerning whether a child's selection was dictated by memory-related as opposed to nonmemory-related factors. The statistical patterns of differences among conditions were identical for the free-response and forced-choice data.

Table 1

Mean Percent Recalled in the Initial Session, by Training Condition, Strategy Type, and List

		Training Condition		
		Strategy Utility	Strategy Affect	Control
A.	List 1 (Baseline)			
	Effective Strategy	39.5	37.1	31.9
	Ineffective Strategy	36.2	36.2	29.0
	$MS_E(120) = 81.8$			
B.	List 2 (Strategy Instruction)			
	Effective Strategy	98.6	96.7	97.1
	Ineffective Strategy	19.0	19.0	9.5
	$MS_E(120) = 23.2$			
C.	List 3 (Immediate Maintenance)			
	Effective Strategy	92.4	89.0	79.5
	Ineffective Strategy	42.9	29.5	29.0
	$MS_E(120) = 106.4$			

Table 2

Percentage of Subjects Giving Different Reasons Associated with Their Strategy Selections on the Immediate Maintenance Trial, by Training Condition and Strategy Type

1. Effective Strategy

	Reason		
Training Condition	Memory	Fun	Easy
Strategy-Utility	100.0	0.0	0.0
Strategy-Affect	0.0	90.5	9.5
Control	0.0	71.4	28.6

2. Ineffective Strategy

	Reason[a]		
Training Condition	Memory	Fun	Easy
Strategy-Utility	76.2	0.0	23.8
Strategy-Affect	4.8	52.4	42.8
Control	4.8	28.6	66.7

[a]Based on the subject's most frequently reported strategy (see text for explanation).